

on November 27, 1872, offers a strong contrast to the comparatively prolonged duration observed at its recent return.

Bristol, December 6

W. F. DENNING

WHILE watching the meteor shower of the 27th ult. I observed what closely resembled the appearance of an aurora. There was seen extending along the horizon from about south to about west-north-west—perhaps further towards the south, for my view was there obstructed—and upwards for about 20° from the horizon, a faint reddish-pink luminous haze, varying fitfully in colour, becoming sometimes nearly white, and in intensity both as regards time and position. The greatest brightness noticed by me was nearly due south. Stars were clearly visible through it.

On referring to the letters in NATURE upon the shower of November 27, 1872—to refresh my memory upon other points—I found that appearances of an aurora on that evening are recorded by “several correspondents.” [Mr. Denning’s letter in NATURE, December 5, by Father Denza in Piedmont (NATURE, December 19), by Mr. Baber at Liverpool (same number), and the Hon. Mr. Newton and Mr. Bruce at Mauritius (“a pulsating coruscation, similar to the appearance of the aurora australis”), NATURE, January 23, 1873]. NATURE for January 16, 1873, contains a letter recording a “pale auroral light” seen at the same time as a shower on December 7, 1838, and Mr. Denning (April 24, 1873), records that the April shower was accompanied by “bright displays of auroræ.”

Mr. Newton and Mr. Bruce add that “the instruments at the Observatory gave no indication of a magnetic disturbance.”

Some of your readers may be able to say whether any magnetic disturbance was observed on the evening of the 27th ult. I saw the auroral appearance about 7.15 p.m.

Rugby, December 7

J. B. HASLAM

P.S.—In a note received to-day in answer to my inquiry, the Superintendent of the Kew Observatory kindly informs me that at Kew the “magnetic curves for horizontal intensity, vertical intensity, and declination were remarkably steady throughout the whole of the 27th and 28th ult., being almost straight lines at the time of the meteoric shower.” He adds that no auroral effects were seen at Kew.—J. B. H. (Dec. 8.)

In case England has been clouded on the 27th, it may be well to state that the meteors were brilliantly seen in the Adriatic. A few were visible on the night of the 26th; on the 27th, at 16h. 30m. G.M.T., they averaged thirty per minute; at 17h. they had much increased, and were counted, at 18h. 10m., at seventy per minute, while at 20h. 40m. they had decreased to thirty per minute again; on the 28th very few were seen. During the rapid shower they were not equally distributed; for six or eight seconds only one or two were to be seen, and then, in a couple of seconds, perhaps eight would be counted, mostly seen simultaneously. The radiant-point was estimated at about 15° S. of the following end of Cassiopeia at 16h. 30m., and at about 3° S. of the preceding end at 20h. 40m. The trails were more persistent and brilliant in the latter part of the evening. One was distinctly seen by two observers to sharply bend its apparent course about 20°, possibly a case of perturbation by a non-luminous meteor, or else of splitting. A large number were as bright as first-magnitude stars, and many equal to Venus.

WM. F. PETRIE

s.s. Tanjore, November 28

FROM the accounts in NATURE and in the Times, it is evident that the display of meteors was much finer in the east of Switzerland than any of those mentioned by your correspondents. My attention was first directed to the shooting-stars shortly after 6 o'clock (local time here being about thirty-eight minutes in advance of Greenwich time). For half an hour after that time the fall was continuous, several meteors appearing together. In fact, so many were falling, that it seemed to me hopeless to attempt to count them, but I should think that they must have fallen, on a moderate computation at that time, at the rate of at least 200 a minute. Many of them were especially brilliant, and those falling near the mountains, which completely encircle this village, produced, I presume by irradiation, the curious appearance of passing between the spectator and the mountains. The richest period of the display when, looking from a window, four or five were seen together in one part of the heavens did not last for more than an hour, but the phe-

nomenon continued with less effect until 9 o'clock, when the sky which, until that time had been perfectly clear, became overcast. The height of the high-lying plateau of the Canton Grisons, more especially in the Engadine, and the remarkable absence of aqueous vapour, causes many more stars to be visible here than in the denser air of England, and this, no doubt, in large measure, accounts for the superior brilliancy of the display as witnessed here. This strangely affected the imagination of some of the peasants of this village, one young woman in particular spent the evening in tears and lamentations, momentarily expecting the end of all things.

J. F. MAIN

Wiesen, Canton Grisons, Switzerland

“Evolution without Natural Selection”

TWO or three points in Mr. Romanes’s letter in your issue of December 3 (p. 100), leave me no other alternative than to again ask you to insert the following few remarks. I beg to inform Mr. Romanes that with Darwinism my book has very little to do. It neither attempts to refute nor confirm the Darwinian hypothesis of Natural Selection. Neither is it an “emendation of Darwinism”; but the facts it contains seem to be an all-necessary supplement to the great naturalist’s hypothesis. It is to be regretted that at the present time so many naturalists accept the theory of natural selection as an exclusive explanation of the evolution of existing species. They unconsciously blind themselves to the existence of any other agent in the work of evolution. To them there can be, nor is, no other. No greater error could be made; and it is my firm conviction that as time goes on the theory of natural selection will gradually lose much of its present presumed universality. What is becoming more evident every day is that existing species do not owe near so much to natural selection for their evolution as extreme Darwinians would have us believe. What the remote ancestors of these species derived from its influence is another matter. How far its influence has been exerted on living forms is not for me even to conjecture; but certainly, so far as birds are concerned, the evidence of its influence is astoundingly slight in comparison with the number of species.

I am very pleased to see that Mr. Romanes has changed his opinion concerning “trivial specific characters,” and now admits that they are both numerous and important. But they cannot even be regarded as “insignificant” as compared with the great “organising work of natural selection.” For, according to the Darwinian theory, they should owe their very presence to its influence, but, unfortunately for the hypothesis, they do not. Once more I must strongly protest against Mr. Romanes saying that my book attempted to explain the cause of variation. It does nothing of the kind. Nor do I consider it fair for Mr. Romanes to infer that isolation, &c., do not explain the cause of variation, and therefore that they fail as evolutionistic agents. It would be just as fair and logical to say that the Darwinian hypothesis is a failure because it does not explain the cause of variation. Darwin must have a variation to begin with for natural selection to work upon; so must isolation. The cause of variation is one of the greatest secrets which Nature still retains in her keeping; but doubtless it will soon be wrested from her.

London, December 6

CHARLES DIXON

I HAVE not changed any of my views; but Mr. Dixon appears to change his within the limits of two consecutive sentences. For, immediately after his strong protest against my statement that he has attempted to explain the causes of variation, he complains of my want of fairness in not acknowledging the adequacy of the “evolutionistic agents” which he has suggested as “the causes of variation.” With this specimen of Mr. Dixon’s method of discussion before them, your readers may be able to sympathise with the failure which seems to have attended my efforts at expounding his essay.

The analogy between isolation and natural selection does not hold. For is it not obvious that while natural selection can be understood to operate in an explicable manner on the variations supplied to it, there is no analogous explanation to be given of the manner in which isolation can so operate—i.e. why isolation *per se* should preserve some of the variations and not others? That isolation is a favourable condition to the occurrence of trivial or non-adaptive specific change, I have not denied; but, on the contrary, expressly affirmed: I have only denied that it can be regarded as the cause of such change—and least of all in any way similar to that in which natural selection may be re-

garded as the cause of important or adaptive specific change. Therefore, if it is the case that "so many naturalists accept the theory of natural selection as an exclusive explanation of the evolution of existing species," I think that Mr. Dixon has done well to correct their error. Only I am not aware that any naturalist of note has allowed his belief in Darwinism thus to go beyond the teaching of Darwin.

GEORGE J. ROMANES

Scandinavian Ice-Flows

FAILING any more direct answer to Sir J. D. Hooker's query (NATURE, vol. xxxiii. p. 79), perhaps, with your usual courtesy, you will allow me space for one or two brief notes. The map referred to, as it stands in "Climate and Time," p. 449, is conjectural to a very large extent. If we are to take the relative closeness of the lines to indicate comparative depth and strength of the glacier-flow, the Baltic must have been, at the intensest period of glaciation, a glacier-filled valley, on an enormous scale, with the ice-stream passing out over the comparatively low, and then submerged, country of Schleswig-Holstein. Dr. Croll, to support a foregone conclusion, represents it thus, and then makes it bifurcate conjecturally about the Dogger Bank. One or two considerations, however, make Dr. Croll's conclusion less "inevitable" than he seems to imagine (p. 449).

(1) Admitting, as we must, that the striations mentioned (p. 448) on the Island of Bornholm, point to the passage of ice in massive proportions over at least that island in the *direction* indicated by the lines on the map, we may still call in question the hypothesis which regards the *main mass of the Baltic ice* as having passed that way.

For (2) the evidence given (p. 449) of its having passed over Denmark (the bare "fact that the surface of the country is strewn with debris derived from the Scandinavian peninsula") is so ambiguous as to be worthless on the point under consideration.

(3) The *facts* stated by Dr. Croll (stripped of the guise with which he has invested them) can be easily and naturally accounted for by the action of *marine* ice, owing its origin to the great Scandinavian glaciers of the period; some of which, even from the Baltic side, probably drifted away into the present North Sea basin.

But (4) that *the main mass* of the ice from the *eastern* slopes of the great glaciated Scandinavian range did not take this direction is proved by some of the best-established facts of European geology; facts which, had they been known to Dr. Croll, would have rendered, I venture to think, the construction by him of the map referred to impossible. On p. 447 he says, "After passing down the Baltic, a portion of the ice would probably move south into the flat plains in the north of Germany, but the *greater portion* would keep in the bed of the Baltic, and of course (*sic*) turn to the right round the south end of Gothland, and then cross over Denmark into the North Sea."

The *naïveté* of this statement, in a book bearing date 1875, is truly refreshing. Any one who knows the district of Jena is perfectly familiar with the enormous abundance of ice-transported material from Scandinavia to be found thereabouts; and these "Findlinge" are spread far and wide over the whole North-German plain as far inland as Bonn, Westphalia, Thüringen, Saxony (even to the south of Zwickau), and, according to Credner's later observations (*Sitzber. der naturforsch. Gesellsch. Leipzig*, 1875), into the interior of Bohemia, as far as Troppau, near the sources of the Oder, on the slopes of the Sudeten Gebirge, and even to Toulou and Moscow.

(5) Lastly, the occurrence of striated blocks of Scandinavian origin in the boulder-clays of the Yorkshire coast, is clearly incompatible with the conjectural view so graphically expressed on the map in question.

A. IRVING

Wellington College, November 30

The Resting Position of the Oyster

I FEEL some disinclination to take up more space in the pages of NATURE on this subject without making any new contribution to the discussion, but Mr. John A. Ryder's letter induces me to summarise the facts which have been brought forward, and the conclusions to be drawn from them. The condition of the oysters examined by me can only be explained by inferring that they were quite free, and resting on a flat bottom with the right valve downwards. The specimens of

Pecten opercularis which I had before me were in the same condition, and doubtless rested in the same position. Of *Pecten maximus* I cannot speak with certainty, and therefore leave to Mr. Arthur Hunt the responsibility of stating that there is a difference in respect of position in the two species. Prof. Möbius also finds that the left valves of oysters are usually covered by fixed animals, but as far as I understand his letter he thinks this does not prove that the left valves in this condition were uppermost: in the oysters I examined, the right valves were so clean that they must have been in close contact with the bottom. The other letters on the subject all describe evidence proving that oyster larvæ attach themselves by the left valve. This I did not deny, and I might of course easily have found the direct testimony of observers on the subject. Mr. Ryder says it is well known that the right valve of the oyster is always the most deeply pigmented, while the left one is paler; in the oysters I examined, the condition of the valves was exactly the reverse of this. It seems to me that when a young oyster is attached to the under-surface of a stone or shell by its left valve its right valve is lower, and if it drops from its attachment, or grows much larger than the stone or shell to which it is fixed, the surface of its right valve will come into close contact with the sea-bottom. I have seen oysters which still retained a piece of shell attached near the umbo of the left valve, while the rest of the valve was covered with fixed animals, and the right valve was quite clean and light in colour. There are no crowded oyster-banks in the Firth of Forth, and it might even be said that the oysters which came under my observation had been dredged and thrown overboard again at some time of their lives. But I do not think oysters are often returned to the water when once taken in the Firth. In my former letter I implied that probably in the normal position of an adult oyster the right valve was in contact with the bottom. That this is often the case when the oyster is free and has plenty of room has not yet been disproved, and therefore I think the current statement that the oyster, when not attached, invariably rests with its left valve downwards needs modification.

J. T. CUNNINGHAM

Scottish Marine Station, November 28

The Sea-Mills at Argostoli

I WILL be glad if, through the columns of your journal, you will be good enough to inform me what has been written in English concerning the phenomenon known as "The Sea-Mills at Argostoli." Having recently visited the island of Cephalonia I was able to examine these mills frequently, and I have reasons for believing that papers have been read at different times at some of the learned societies at home discussing the subject *in extenso*. I will therefore be glad of *any* intelligence on this interesting phenomenon which you may be able to refer me to.

J. LLOYD THOMAS

H.M.S. *Téméraire*, Mediterranean, November 15

Earthquake

ON Thursday morning, December 3, I was in bed awake, between 6 and 7 o'clock. I heard a slight clattering noise of the earthenware and glass articles on the marble top of the washstand. It lasted for about three seconds, and went with a slight halt near the end. There was no one moving in the house, and nothing outside to cause the tremors, which I did not feel in bed. I immediately got up to look at the clock, and found it was 6.45. I do not know at what time the earthquake in Algeria took place, possibly there was some connection between the two, and the tremor may have been felt in other places in England, so I record this.

Gateshead, December 6

R. S. NEWALL

VENTILATION

IN modern life, with its enormous populations living under artificial conditions in towns and cities, the subject of ventilation, or the supply of sufficient pure air to each individual for the maintenance of health, has assumed, as it has become more generally understood, a vast and national importance. Its importance has been clearly demonstrated in many instances by a greatly diminished death-rate in places where overcrowding on space or in houses, formerly existent, has been remedied,